when in a horizontal position. In order to reach some definite results, glass rods of different diameters were used. They were so arranged as to allow an increase of their weights by attachments, and so that the hand might be applied in a perpendicular position. When first examined, on Sept. 22, it was found that the extreme limit of weight which could be made to adhere, by means of a glass rod of 10 millimetres diameter, to the surface of the front part of the four fingers of the right hand, when held perpendicularly, was 1,450 grams. A glass tube of 20 millimetres diameter was next substituted, and would yet adhere when its weight had been increased to 1,900 grams.

When the experiments were repeated on subsequent days, the same glass tube could each time be loaded heavier, and Mr. H. can now lift the comparatively enormous weight of 2,610 grams, after having pressed his fingers tightly to the glass rod, which stands in a perpendicular position upon a metallic disk to which it is fastened, and which also carries the weights.

I ought to state that the thumb is never used to cause the adhesion, and that, in commencing a series of experiments, Mr. H. can never at the beginning lift the greatest weight. It appears that the power of adhesion increases during a series of experiments made within a period of fifteen or twenty minutes. So far, the power has continued to increase almost from day to day, but appears to have now reached its maximum. Following are the results of a few of the experiments made as described above; the first figure representing the diameter of the glass tube (in millimetres), and the second the maximum weight suspended (in grams): 5, 1,530; 10, 2,120; 15, 2,400; 20, 2,610; 25, 2,260; 30, 1,860.

The weights recorded above are nearly one hundred times greater than those which can be lifted by adhesion when the corresponding tubes are used horizontally. Exact measurements of that portion of the hand's surface which comes in contact with the adhering mass are difficult to make. However, the determinations were made sufficiently accurately to show that very nearly 3 square centimetres surface enter into action during the adherence of a 20-millimetre rod, when supporting 2,500 grams.

When the investigation was first begun, Mr. H. not only firmly believed in his utter inability to use his left hand as he did his right, but also looked for the seat of the adhesive power only in the front part of his fingers. It has now been demonstrated that the left hand does all the work equally as well as the right one, and that the surface of adhesion extends, though different in intensity, over almost the whole of the inner part of the hands. The power is strongest in the front part of the fingers, and weakest in the centre of the palm and on that portion of the fingers which is nearest to it. All the protruding portions, including the ball below the thumb, possess adhesive power, however. Neither the back of the hands, nor other parts of the body, including the surface of the soles and toes, show any signs of adhesion. The power of the hand to sustain objects may be shown by suspending upon it, for instance, four 6-inch test-tubes alongside of one another, or by applying an iron rod, a wooden stick, and a glass tube simultaneously to different parts of the hand. A test-tube adhering to the hand may be made to roll to and fro by jerking the hand backward and forward while the tube is in a perpendicular position.

The intensity of adhesive power in the various fingers differs widely. It is strongest in the index and middle finger, and weakest in the little finger; the latter doing so little work, that the three others may lift almost as much as the four. What is most singular is that one finger possesses very little power. The greatest weight shown to adhere to one finger has been about 35 grams, while two fingers may lift 1,400 grams. In order to decide whether or not the aid given by a second or third finger, in balancing or steadying the weight of the suspended mass, was the cause of this inability of one finger to do much work, three fingers were covered with a thin film of collodion, which rendered them unfit to act by adhesion, but not by their muscular support.

The experiments thus performed showed conclusively that the three fingers covered with collodion were absolutely unable to assist the fourth one. It can therefore not be the steadying power which causes two fingers to do forty times the work of one finger. That this should be so, might have been inferred from the fact that Mr. H. can suspend a combustion tube about four feet long on two fingers, and cause it to swing like a pendulum through a distance of at least three feet.

The length of time during wnich substances adhere depends chiefly upon their weight. Light objects, such as test-tubes, will remain suspended even horizontally for ten minutes or longer, and can then be removed only by the application of some force, when a slight click, caused by the concussion of air, can be heard. Very heavy articles will fall off sooner; but whether in consequence of a diminution in the adhesive power of the surface, or in consequence of the strain exerted upon the muscles, it is difficult to say. Another cause of the falling-off is to be found in the perspiration which at times oozes freely from the pores, and interferes greatly with the experiments.

It may be added, that neither the shape of Mr. H.'s hands nor the structure of the skin, even when examined under a magnifying-glass, shows any thing abnormal, though the skin is very soft and smooth. These are the principal results of the investigation made, and the next question is, how to account for the phenomenon. I need not mention the reasons which exclude the possibility of an electric or a magnetic action, because the facts presented show this conclusively. We therefore seem to be limited to a consideration of surface action, or atmospheric pressure, or both. The reasons for this assumption are, (1) that it has been found impossible to notice any attraction whatever exerted at a distance; (2) that the power increases with the cleanliness and smoothness of the surface, i.e., with the number of actual points of contact; (3) that the peculiar sound heard on breaking contact is characteristic of the concussion of air; (4) that the power increases with the increase of surfaces in contact, as shown in the experiments with glass tubes of different diameters.[1]

Whether, or to what extent, the pressure of atmospheric air induces these phenomena, I am unable to say. I have not had an opportunity to examine Mr. H. under a diminished or increased pressure, but hope to do so ere long. Certain it is, that the ratio of one square inch of adhering surface to fifteen pounds in suspended weight has not been exceeded, though approached to within twenty per cent. But even if air-pressure participates, as it most likely does, we have to assume that the skin of Mr. H. is peculiarly fitted to show these phenomena of skin-adhesion, and in a degree, to my knowledge, unnoticed heretofore. That he is not the only person possessing this power, I have good reason to believe. Among a large number of people examined, there were many whose hands showed at least signs of this power, and certainly a few who promised to develop it sufficiently to exclude doubt in regard to the occasional existence of the force. It may be well to warn persons who may try experiments, not to mistake for actual adhesion the suspension of tubes by means of counter-pressure exerted by portions of the terminal phalanges or the fleshy portions surrounding them. The unmistakable sign of adhesion is the performance of the experiments with the fingers kept absolutely close to one another, in which case it becomes next to impossible to exert counter-pressure. That muscular action may come into play in some of Mr. H.'s experiments is not absolutely impossible, yet very doubtful. I leave it to physiologists to furnish a more satisfactory explanation of these phenomena than I myself have so far been able to give.

W. SIMON, PH.D.

Baltimore, Dec. 16.

Convectional Currents in Storms.

READERS of *Science* will remember, that, in the numbers for May 10 and June 21 of the current year, there were given some computations of the probable effect of convectional currents and of the condensation of moisture carried by them into the cooler air strata above. These computations showed that there could be no liberation of energy from any such action. An interesting article has appeared in the *American Meteorological Journal* for December from the pen of Professor Davis of Harvard University, in which I find, "It is difficult to understand why this question should be so confused by Hazen, as appears in his recent articles. . . .

[1] That there is a decrease in power when the tubes are wider than 20 millimetres may be explained by the fact that those surfaces of the fingers which show the highest degree of adhesion are prevented from coming in proper contact with the surface of tubes, when of too large a diameter. This would account also for the poor adhesion of objects with flat surfaces.

It is a mistake to say that latent heat thus liberated [from the condensed moisture] will warm the air enough to allow the condensed vapor to evaporate again; for the latent heat is completely expended in the work of pushing away the air that surrounds the ascending expanding mass, and therefore cannot be applied to any other task. Espy made this error for a time, but afterwards corrected himself. It is regrettable to see the error now revived by Hazen." As all these computations were based on commonly accepted theories, it is a little difficult to comprehend these expressions. I hope to show that the confusion is where it is least suspected. I am aware that the ordinary theories have such a fascination, there is little hope in bringing the philosophers who accept them to what the facts seem to indicate; but there are an increasingly larger number of persons who have grave doubts as to the sufficiency of present meteorological inferences to account for the facts observed, and it is for these I write, as well as to explain my position.

The fact that there is no exchange of air *en masse* from one level to another has been proved by the strongest arguments, and such as have not been controverted. It is a great pity that this assumption should be boldly made at this day without answering the objections. It seems high time to lay aside "glittering generalities," and carry out our analyses to the actual conditions we observe. To do this it is only necessary to set forth quantitative computations of the effects produced by certain inferences. I am not aware that this has been attempted save once (see *Science*, xiii. p. 369). In that case the assumptions and results were so absurd and so easily controverted, that it is not surprising that no philosopher has taken up that line since.

There are two points to be made plain. First, regarding "work." This is the great shibboleth of theoreticians. If there is a troublesome quantity of heat to get rid of, or the formation of vapor which is the source of energy to account for, "work," and the difficulty disappears. If we place *a* pounds of gunpowder in a cannon, and discharge a ball upward, a certain number (say, *b*) of foot-pounds of work is done, and this can be definitely computed. If a similar amount of gunpowder be strewed over a field, there is, what we may call, the same potential energy present as before; but no one believes that firing the powder will carry a shot, or that a single foot-pound of effective work will be done by it. This would appear one of the most serious defects in modern theories. The philosopher sits down, draws on his thinking-cap, and, seeing rain falling at the rate of two inches per day, in a twinkling finds that 11,796,000 cubic inches of water is condensing over a single acre. Next he finds a million times that in an ordinary storm, and this represents billions on billions of foot-pounds of energy. Is not this the veriest nonsense? What these theoreticians need most of all is to transport their steam-engine, if they can find one, into the cloud region, and then compute the amount of work actually made effective. No one, outside of these philosophers, would boil away tons of water in the open air on the Atlantic coast, and imagine by this means to obtain effective energy enough to transport a great steamer across the ocean in less than six days.

A word regarding the "using-up of the latent heat of condensation by doing work in pushing aside the air which surrounds the ascending expanding mass." Nothing can better illustrate the views just enunciated than this inference. It is universally accepted that air blows toward our storms almost normally to the isobars at the outside, but more and more at an angle as it approaches the centre, till it becomes tangential at ten to two hundred miles from the centre. This whirling column has, it is also inferred, an upward convectional movement at the centre. It is impossible for us to imagine that there is a central core, forming a convectional current, and that on all sides of this there is a vertical cylinder of air pressing in on the core, and which must be pushed aside; for just beyond this core the air is whirling in the same circle, and it is believed by some that the centrifugal effect would even throw this outward. This shows conclusively that there is absolutely no air to push aside, and, even if there were, the work needed to move it in a frictionless medium would be inappreciable. Is not this inference a most weak attempt to bolster up an exceedingly weak theory?

It is probable that the old inference that the sun heats up a limited portion of the earth's surface, and sets up a convectional current which ultimately results in a violent tornado, will soon disappear. Professor Ferrel, one of the most ardent advocates of this inference, has recently declared, that, in order that this convectional current may not be broken up from the greater speed of the upper portion, it is necessary to suppose that the upper part separates from the lower, advances in front of the storm, and sends its gyrations through a frictionless medium to the earth. Verily, to use a homely but forcible and apt expression, "this is cutting off the tail of this theory close behind its ears."

It is now known that the sun's heat has no direct effect upon air-columns near the earth. We know,

1st, That the earth becomes very hot, but the air is almost a non-conductor of this heat; and this effect extends only a few inches.

2d, That convectional currents occur only between contiguous air strata, and there is no transport of air *en masse* by them.

3d, On some days the air is heated thirty or more degrees above the morning temperature; but this produces no effect on the moisture contents of the air, it does not produce any convectional current, and the heat extends over a circle about a thousand miles in diameter.

4th, As a storm approaches, clouds cover the sky, and the direct effect of the sun's heat is almost entirely removed.

5th, Notwithstanding the removal of the sun's direct influence, the moisture in the air is most remarkably affected. We find enormous additions to this moisture over a region extending for hundreds of square miles in front of the storm. Whence comes this moisture? We have indubitable evidence that heat has nothing to do with it. Its occurrence is entirely independent of the winds. It does not descend from above, for there is ordinarily less moisture there than below, and theory indicates an upward and not downward motion. It seems to me this is one of the most important points to be determined. It would seem that the moisture collects in the upper regions before the storm, for the first indication of the storm is the high cirrus four hundred or five hundred miles in advance. This shows plainly that the origin of the storm is not from convectional currents beginning at the earth's surface. Is there a condition in the atmosphere which is so changed upon the approach of a storm that the air begins to absorb moisture? Is there an influence from the sun that only requires a slight change on the advance of a storm to cause the moisture to mass itself? Is there a condition in front of the storm itself that attracts moisture directly without its transport by air or heat currents? Does the moisture come from the whole region near the storm, and mass itself at it? These are startling hypotheses, but they have much to support them. Our storms come over the arid plains of the West with little moisture in them. Almost suddenly, as they approach the more fertile valleys, there is a marked increase in the moisture. Light rain begins, which becomes heavier the farther east the storm moves. At times the storms move clear across the country without depositing much moisture. Is this because the attractive force has less power, or because it holds the moisture more tenaciously, or because the air is too dry to allow precipitation? We have here what seems a most important field of research, and one that promises much. H. A. HAZEN.

Washington, Dec. 13.

INDUSTRIAL NOTES.

Calendars.

AT this season, when every one is looking for a convenient calendar for use during the coming year, there should not be overlooked the various very attractive calendars, issued as advertisements, it is true, but in which the advertising feature is not introduced in a way to make the calendar objectionable. Among these we have just seen that issued by the well-known firm of C. I. Hood & Co., of sarsaparilla fame. This calendar can be had for the asking at any druggist's, or is sent postpaid on receipt of 6 cents in stamps at the main office of the firm in Lowell, Mass. The head of a young girl lithographed in fourteen colors appears on the face of the card, and is an admirable example of what can be done in this line of art. It is a very pretty bit of color to brighten up some dark spot.